

## Round Table Discussion: Science in a Political World

R.V. Stachnik<sup>1</sup>, and D.M. Peterson<sup>2</sup>, moderators

*Panelists:* D.M. Peterson<sup>2</sup>, R.J. Allen<sup>3</sup>, C.A. Beichman<sup>4</sup>,  
K.J. Johnston<sup>5</sup>, S. Kulkarni<sup>6</sup>, A. Labeyrie<sup>7</sup>, M. Shao<sup>4</sup>,  
and G.W. van Citters<sup>8</sup>

**Abstract.** The conference *Working on the Fringe* ended with a roundtable discussion of the bigger picture of what it takes to ensure science gets done, in this case optical and IR interferometry. Every scientific field requires leaders and visionaries who lay the groundwork for new instruments and observatories on which the growth of an observational science depends. The panelists shared their insight into every aspect of the process of making science happen.

This transcript was made from tapes provided by Dan Gezari. The tapes only captured part of the contributions from the audience, but editing has been limited to correcting errors in transcription. Bob Stachnik introduced the Panel members, and moderated the first half of the roundtable discussion. Deane Peterson led the second half.

*Stachnik:* I'd like to ask the panel a series of questions. These have been winnowed down to a mere 15. We have an hour, leaving 4 minutes per question, so we'll just see how we progress through these. The first question I have for anybody on the panel is, what are the realities of getting a big space mission or large ground interferometer approved and where does politics enter the picture? There's nobody here from NASA Headquarters, by the way.

*Anon:* Chas, go for it!

*Beichman:* One thing I did in the last 6 months is gave two talks in high energy physics departments; one a conference, and one a colloquium. The high energy physicists invited me to come because they said "we are so envious of you guys in astronomy and at NASA because you have these big projects, billions of dollars, many hundreds of millions of dollars... How do you do it?" And I think it does come to this issue when you try to get a big space mission, you do have to

<sup>1</sup>Testex Scientific, 8 Fox Lane, Newark, DE 19711

<sup>2</sup>Department of Physics and Astronomy, SUNY, Stony Brook, NY 11794-3800

<sup>3</sup>Space Telescope Science Institute, 3700 San Martin Dr., Baltimore, MD 21218

<sup>4</sup>Jet Propulsion Laboratory, California Institute of Technology, Pasadena, CA 91109

<sup>5</sup>US Naval Observatory, 3450 Massachusetts Avenue NW, Washington, DC 20392, USA

<sup>6</sup>Department of Geological and Planetary Sciences, California Institute of Technology, 1201 E. California Blvd., Pasadena, CA 91125

<sup>7</sup>College de France, 04870, Saint Michel l'Observatoire, France

<sup>8</sup>Division of Astronomical Sciences, National Science Foundation, 4201 Wilson Blvd, Arlington, VA 22230

convince the taxpayer, or their representatives, or Congressional staffers, that you're actually addressing a question that they care about.

I think one of the great attributes Dan Goldin has as NASA Administrator is the understanding that the era when you just went with some National Academy study (basically just "carried out by your buddies") that said you ought to get a billion dollars to get this particle accelerator, those days are gone. The Cold War imperatives, or whatever, that made something or other a routine, "straight to the top of the list", "you get your billion dollars", Project, are gone. The dollar amounts we're now talking about are so great that if you're going to convince people your project is worthwhile, you actually have to answer a question people care about. And I think this whole question of finding earth-like planets, for example, is among those. Astronomy, in general, is very lucky in that it produces pictures that look good above the fold in the New York Times. Those pictures are also relatively explainable, whether they portray early galaxies or gamma ray bursts. Also, big bangs are good. Explosions are good, people like them. Very big explosions are still better. But you know, little squiggles in a cloud chamber are not so good, as our physicist colleagues have found out.

Spectra are a tough sell, too, and Dan Golden understands this. That's why high-resolution spectroscopy for a TPF follow-on is not what he wants. He wants LANDSAT pictures! He understands pictures sell. I think the important thing for a mission is to come up with a very compelling scientific question, and I think planets are probably the biggest problem around, when you're asking for a couple billion dollars. You know, if you're putting in for a \$50,000 grant, galaxy butterfly collecting may be a very good thing to sell to your colleagues. Above a certain level, you know, when you get the National Science Board, or whatever it is within the NSF, at the OMB (Office of Management and Budget) level, you have to have something that real people want to have the answer to, not just your colleague down the hall.

*Stachnik:* Chas, do you think that the Origins Program would have been as saleable if the Administrator was someone other than Goldin?

*Beichman:* I think Goldin really motivated the community to come up with something saleable. And at these levels, it is a marketing issue. He figured, what could he get that was scientifically compelling, technologically challenging, and that the science community would buy into? So I think he had a great deal to do with the origins of Origins.

*Van Citters:* Let me echo some of those statements from the point of view of National Science Foundation. One thing about NSF is, of course, that we support a whole range of basic science from essential, non-clinical biology, to chemistry, to materials research, and so forth. And so, realistically, if a large project is to get going, even within the agency it has to have broad support. You have to have scientific questions, compelling scientific questions, that are going to have an enormous appeal at all levels of the Foundation, including the Assistant Directors, who tend to be a bit like like Heads of Schools within a large University. So this sort of killer-scientific-case, is an absolute necessity just to get it out of the Foundation and then into Congress where you still have to sell it. This is an extremely important point.

*Allen:* Well, I was just musing, as Chas was talking, about the boundary between when a project is so expensive that you have to be assured of catching the



Panelists from left to right: Ken Johnston, Mike Shao, Wayne van Citters, Ron Allen, Shri Kulkarni, Chas Beichman, Antoine Labeyrie and Deane Peterson

fancy of the public and when a project is below that threshold, where the fancy of your colleagues and professional astronomers is adequate. I was wondering if there's any feeling for where a large ground-based interferometer might fall. Because I think the science case for a large ground-based interferometer might very well be made to our colleagues in research and astronomy, but it's not clear to me that the public would grasp it in the way that they've grasped the goals of TPF.

*Van Citters:* I guess... I'd have to think about this a little... with the NSF, the salability of projects within the major research equipment account does depend on some interaction with Congressional committees and staffers. But they are at such a level, or at least historically have been at such a level, that they can be handled within funding consistent with the level-of-effort funding from the Major Research Equipment account with the Congress. So if it's a \$500 million project, and we historically have had \$300 million ongoing projects, and we spread it over a couple of years, it's not something that you'd have to go out and sell to every taxpayer on the street. The first and foremost thing is that you sell the astronomical community on the value of a major effort to their own research and have them advocate it strongly within the Foundation. When that community comes to Washington to talk to their Representatives, they typically carry their message forward.

*Stachnik:* Let me ask you another question. Who are the major constituents for a big project or mission? What's the relative importance of keeping those different constituents happy?

*Kulkarni:* I think for big projects, I'm not sure I can speak of the billion dollar things (there is limited data, there are very few) but at the few hundred million dollar level, we have a reasonable historical track record to evaluate how projects are conceived and how they eventually come to realization. At that level, you really have to build up support across a community. You have to

sell such a project to your stellar astronomer, your interstellar medium person, and so on and so forth. An example is the VLA which was sold on a variety of grounds, and more recently the MMA. I remember 10 years ago there were all these university-based groups that, with difficulty, were detecting CO in nearby galaxies and disks around certain stars in our galaxy. That looked interesting but when the sensitivity of these instruments became significantly better so that we could look at dust in distant galaxies, look at Keplerian disks, suddenly the whole universe opened up. I think a phase transition occurred when the MMA went from just being the preserve of millimeter astronomers to the preserve all of us. So I think in a large project it's essential that you show great and rich returns.

*Stachnik:* You're talking about our scientific constituencies, but there are also the people in the funding agencies and frankly in the Congress as well. There are people to sell to there, as well.

*Kulkarni:* Right. Restricting oneself to the \$100 to \$200 million things, where the scientific grassroots work has to be done first. At that level, I think, it works more or less the way I said and the MMA is an excellent example of how this process worked.

*Stachnik:* There are people like Wayne and Chas, or perhaps Mike and Ken, who might have insights into selling a project beyond the scientific community.

*Van Citters:* As Shri said, the first thing is to build a broad constituency within the community. A large project which has a very small constituency within the ground-based astronomical community is probably not going to go very far because in the end it has to have the advocacy of the entire community the way the Millimeter Array did. And as Shri said, it took a long time to build up from the millimeter groups at Owens Valley, and then Berkeley, and then BIMA, into the Millimeter Array. So building a constituency is the first step.

But there is another step, at which we compete with physicists and chemists and biological scientists to get our ideas before the National Science Board for an endorsement and then into our budget submissions to be sold on the Hill. It's extremely important that one's constituency be broad so that our advocates can undertake their very informed and agile advocacy for a project in front of diverse groups and succeed in that advocacy.

Our community needs to understand the processes the Foundation goes through in order to get something funded. The community also needs to understand when to put its foot in and when to stay out. After the Foundation puts something into its budget for, for instance, major research equipment requests, it's probably far better for the community to support the overall Foundation request than to lobby for individual projects. Inevitably, non-advocates seek to promote discussions about "Why this project rather than that one?" Quite often science as a whole loses in that sort of discussion. In an instant, the unspoken assumption of a zero-sum game has been introduced and the advocate has been co-opted into providing arguments in support of that notion. One project or another may win in the short term but science loses in the long term.

So the first thing to do is to convince the agency to put your project into its budget and then support the overall agency budget when it goes to the Hill. Go to the Hill, talk to the staffers, but talk to them from the point of view of

advocating your entire science. That's the thing you best understand. Then point out how a project you favor will help the broader field.

*Beichman:* I would like to return to this question of the threshold at which different criteria come into play. From the NASA side, I think there is at least one fairly clear line. Explorer class missions and below are purely peer-reviewed activities, equivalent to the NSF's proposal-driven, not mission-driven, projects. From that program you can get up to something like \$140 - 150 million for a project and you don't have to convince anyone other than your peers that your idea is better than theirs. It's intensely competitive but it really is science-idea driven and there's no new start required at the Congressional level. There are Explorer class opportunities that come and go every few years as part of a level of effort activity.

At NASA, you cross that next threshold when you want to get a new start for something bigger than an Explorer, like the Next Generation Space Telescope, or a large submillimeter array, or x-ray telescope, or what have you. At that point, you do need to lobby within NASA, to get into NASA's plan. Then, when you actually get to the point where you're trying to convince the OMB and then Congress to get your project done, there is significant public advocacy that has to happen. Individual scientists go to their Congresspeople.

We saw this on SIRTf. Marcia Rieke did a spectacular job of mobilizing scientists in key Congressional districts to drop in on their congressmen. That's an important aspect above the Explorer level.

*Stachnik:* I want to underscore Chas' point about the existence of very different kinds of missions. On the one hand, there are those which are Explorer class or smaller. For them, the money is there every year and the OK to start work on such a mission results from straight scientific review combined with the Agency's consideration of its programmatic needs, such as what NASA needs to fly now in order to pave the way for future missions. Quite separate are 'New Starts', which require additional ('new') money or major reallocations of dollars. It is worth stressing that this is an entirely different kind of process and it has visibility at the level of the Congress.

Let me ask another question...(Request from the floor to ask a question)...uh, yeah, sure!

*Johnston:* Go ahead! You're our constituents! (laughter)

*Andrew Gould (from the audience with no microphone):* I'd like to hear a concrete assessment of SIM as a project which seems to have a lot of momentum, not coming out of the scientific community or broadbased at all as far as I can tell, but driven more or less by a scientific rationale that's coming more from NASA, which seems completely contrary to the model that's being proposed here...

*Peterson:* I think I raised my hand first! I take exception to the 'lack of broadbased support'! The concept of SIM has been around since the late 70s and it's been reviewed and re-reviewed. It has a justification that is extremely broadbased and has been evaluated by a lot of different review committees.

*Stachnik:* Including the National Academy, which sets our global priorities.

*Beichman:* That's right, and it came out of the Bahcall Report as the highest priority among the moderate class missions.

*Gould:* I'm just telling you about the reality, as perceived by most people: SIM is barely in their consciousness, it's coming out in sublevel reviews, but it does not have the broadbased support that you guys think that it does. I think it could galvanize it. It is galvanizing it, but...

*Peterson:* We're paying more attention to raising consciousness on what SIM can do. Maybe we need to do more. It turns out it's a very easy sell. It's really just a matter of getting the time in front of each of the interested groups.

*Stachnik:* If I could make a point. Some years ago astronomy really got its act together and embraced the National Academy Review Process, which has been the envy of other scientific disciplines and has increasingly been adopted by them. Both Congress and the funding agencies also love it for giving them a vetted, consensus set of priorities, rather than forcing them to confront a bunch of people coming at them from different directions. In that latter case, the best sales job to a non-scientist Congressperson is the one that succeeds. It was the more disciplined National Academy review process that gave us SIM.

*Peterson:* Let me add Andy, I know you are in fact quite for SIM, so this comment is not aimed at you, as supporter of SIM, but it's clear to me that Ohio State needs a lecture, can I offer my services?

*Gould:* Ah...yes... (laughs)

*Shao:* Yes, I think on the other hand, I think Andy is correct in that, despite the fact that SIM had a National Academy recommendation, almost 9 years ago, it really took Dan Goldin's getting into office to make the mission happen. Actually, even more than Dan Goldin getting into office, was the fact that back in 1996 and 1997 the NASA budget was looking at a 10-15 % per year decline such that by the year 2000 or 2001 the space science budget would have been one-half of what it was in 1995. It is the fact that the economy turned around and Dan Goldin came in, that's what really opened up a wedge for the whole Origins Program. So some luck is involved. But a lot of the groundwork was laid by the National Academy report.

*Kulkarni:* Yes, I think Andy's sense is actually correct. I would like to say that because the way SIM succeeded is because it's a part of the larger scheme of things. Chas' talk had the elements clearly laid out. I think that without the sort of grand goal SIM has, it would be a little harder to sell because the astrometry community is so small. But there's another effect here which I saw at work with VLBI. I remember very distinctly when I started doing my thesis work in VLBI. The VLA, the way it was sold and constructed, is completely opposite to what we are discussing right now. VLBI is done by a small number – a very small number – of astronomers. The topics it can attack are, by its very nature, limited, much like SIM. It can do certain things very, very well, but it can't do everything for everyone. like SIRTf, for example, or Keck, or Gemini. I think SIM has the other thing which I think can also help you win. If you have a lot of coherence within the community (and the VLBI and radio guys are actually very coherent) you can do very well in getting large projects started. For a community of their small size they have done much better than optical astronomers who are not so coherent. Look at how much more funding they get per capita. For a radio astronomer it is an order of magnitude larger

than for optical. So I think that coherence in a community can take you a long way.

*Johnston:* Well, Andy, I share your viewpoint in a lot of respects. I think the major thing for any of these large projects is advocacy somewhere in the government itself. Also, looking as a purely jaundiced person who's outside of NASA, you have to find something for your laboratory to do. SIM is clearly a very good thing to do. The JPL Director will say, "JPL is looking for good projects." SIM comes along and he says, "This is a good area for us to do research in." So it's really important to get a place like JPL, or for radio astronomers, NRAO, to adopt and advocate your program and actually carry it forward. If that advocacy is not there, it'll never go anywhere. Then you have to establish the advocacy, within a larger agency like NSF or NASA, then go on to Congress and sell it there. But without having some agency really put the project together for you, it won't go anywhere.

*Peterson:* In fact, going all the way up one more tree instead of back to Andy's original question, there was one other point. SIM did not look like it was going to survive at one point. We had essentially been told that they were going to close the door on us, and suddenly a planet was discovered. Suddenly they realized they needed to do their interferometry and they had an interferometer sitting on the side, not knowing what to do with it, and suddenly you just could not kill SIM. That was the history of it. I'm not sure if there is a lesson to learn. It was such an unusual sequence of events that I doubt it'll be repeated in our lifetime. I mean, it just happened that way.

*Stachnik:* As the level of candor is rising, I'd like to move on to the next question, which is: What are your perspectives on dealing with NASA Headquarters and NSF? Again, there's nobody from Headquarters here. Anybody? Anybody at all?

*Peterson:* Bob, why don't you jump in there?

*Stachnik:* Ah, I've got my own thoughts on this but I want to see what you guys think.

*Shao:* Well, I haven't dealt with NSF in many, many years of course, but actually interferometry got started with the NSF, a long, long time ago. All my dealings with the NSF have always been very positive. If you're at the small grant level, things work very well at the NSF. You simply submit your proposal and, if it's properly reviewed, it just gets funded and (laughter).

*Beichman:* That was a while ago! (laughs)

*Johnston:* Wasn't that about 20 % funded? (laughs)

*Shao:* 20 %? (laughs) I guess, yeah, it's about 20 %. That's a tough ratio to work against. But as a project gets bigger, that's when things became a lot harder.

*Stachnik:* I can tell you about how NASA Headquarters works. I worked at Headquarters for 10 years. Peer review is a given. It winnows the choices down to a small number of often very different kinds of science. But then Headquarters works internally very much the way a large corporation or a national center, or a university works when big decisions are to be made. This is true in the sense that the decision chain often consists of a very, very small number of people, and the directions in which the organization turns, its scientific preferences, can be

determined by the judgments of that very small number of individuals. Optical interferometry got only modest attention until Dan Goldin showed up. Goldin brought with him a different perspective on the world and an inclination toward Grand Visions. He loved interferometry. There's that element of randomness again. But there is also the point, in considering the current (Goldin) era, of paying attention to management's predispositions and of responding to them.

*Van Citters:* Let me expand on that for a minute. A strong scientific case and a strong technological base for a large project are certainly necessary, but not sufficient, conditions. I go back to the history of large telescopes, in particular, to national programs for large telescopes and the genesis of Gemini back in 1989. There had been a lot of activity within the astronomical community in support of 8-10 meter telescopes. There had been technology development, and proposals written and yet an NSF-funded telescope was going absolutely nowhere. The technology was there. It was clear that it could be done. We even knew the price tag, but it just wasn't catching on. It took Eric Block, whatever else one might say about his vision of science, to get this gleam in his eye that this would be a splendid example of an international project for the Foundation to hang its hat on and take to Congress. That was the birth of Gemini. And it was basically one man's decision that brought it about.

*Stachnik:* Wayne, that's a great lead-in for the next question, which is: What are the realities of international collaboration from the US perspective, both pros and cons?

*Johnston:* Not this year! (laughter).

*Van Citters:* Well, let me speak just from the perspective of our experience with Gemini and our whole building experience with ALMA in cooperation with ESO and PPARC and so on. The current reality is that a ground-based interferometer project of the magnitude we're talking about would have to be an international collaboration. It's not likely that the Foundation would be able to come up with the sort of resources necessary to undertake it unilaterally. And there's also a view, which is absolutely true, that the US does not have a stranglehold on technology and scientific innovation. One really ought to maximize the capabilities and the resources that are put to solving problems and, in general, this favors international collaboration.

*Stachnik:* Very sage and diplomatic. Are there any cons?

*Van Citters:* The cons? Well, one con is that the scientific community has to realize that total control would not rest with the US community. On the other hand, you do get a lot of extremely interesting input and capability brought to bear. It's part of the con that the benefits of an international project do not come without cost. It's a lot of effort on our part, a lot of effort on our partners' parts. A great deal of education has to go on within the US agency, and within foreign agencies, to make a project a success. It's a hackneyed phrase, but a lot of team building and trust building needs to take place. Everyone needs to understand that we're all after the same scientific goals and that the other guy is not just trying to siphon money from your agency.

*Beichman:* When you ask what the realities of international collaboration are, several timescales need to be considered. I think that on the longest scale, international collaborations have proven to be immensely rewarding. If you look

at NASA and NSF, there are excellent histories of international collaboration. But some elements are reminiscent of weather and some of climate. The long-term climate for international collaboration is good but, at the moment, the weather is terrible. I commend to your attention to the text of the Cox Report (China - US espionage) that was in yesterday's New York Times, if you want to see what the weather report is for international collaborations in high-tech areas. It is going to be very difficult to get collaborations over the next few years given concerns about transfer of sensitive space technologies. Antoine and I were discussing the fact that even if you are gaining technology from another country, there may well be domestic barriers to that. It's impedance and it goes in both directions. That's going to be a downside for the US. The US does not have a stranglehold on these technologies but there are people in Congress who certainly think we do. Some believe that all technology flows from the US outwards. There may be some areas in which that's the case, but not very many. I think that view will be to our detriment. In any case, there are very real regulations in place that are going to control what sort of things, at least for the next few years, we are going to be able to do.

*Labeyrie:* I was going to ask Dr. Van Citters whether there is anything like an international club of agencies, which has the brief to define common goals and decide coordinated funding strategies?

*Van Citters:* Yes there is, in a number of different guises. It's not an official organization, I guess, but the Ministers of Science of the G7 Nations meet regularly. Gemini was one of the initiatives that came out of that. Both the OECD (Organization for Economic Cooperation and Development) and, I think, the G7 Ministers have come to the conclusion that it's best if a mission's scientific rationale come out of their scientific communities and not impose it from the ministerial level.

*Stachnik:* If I might make a comment on my own, again from the NASA Headquarters' perspective. There were people I worked with who couldn't stand international collaborations. One of the reasons was the fact that they saw them as bringing undue complexity to missions. One partner, for example, would not be able to provide funds on the expected timescale, which caused your (or his) marching army to continue marching, extremely expensively, at full strength while you wait for the other guy to come up with funds, or to solve a technical problem. There were also issues of integration between groups working 3-5,000 miles apart. There are reasons, legitimate technical reasons, for resistance to international collaboration in contrast to the obvious cost advantages.

I would also point out that, in promoting any sort of collaboration, what agency management tends to look for is not someone who comes into your office to say "Hey look, here are 6 things I can do for you which you didn't know you needed." What's really helpful is if you've got 6 things you are terribly worried about and somebody comes in and says "I will solve one of those 6 problems." That sort of introduction will guarantee you a better response at, say, NASA Headquarters or JPL. This is another way of saying that customer service is important to everyone. You want to bear in mind what it is the guy you are trying to sell something to actually wants and needs.

*Allen:* I just wanted to connect to a point you made. You were describing a situation where multi-national collaboration gets into trouble because one of the

partners can't come up with the money in time and the other partners are chafing at the bit. In particular, the example you suggested was that the US partner would have the money and be chafing at the bit waiting for some European partner to come up with the dollars. But there's a very strong advantage to those international collaborations which I think ESO has successfully exploited over the years. That is that it's very hard to kill these projects. So even though a particular nation or a participant might have trouble coming up with the money because of transient internal problems, the project nevertheless will go on and that their participation cannot be removed. I think, as I said, ESO has been very successful at making sure that their large projects go forward in this way, so I think that's a big advantage to international collaboration.

*Audience member:* In some countries there's a person designated to be responsible for driving development of technology or design. Israel is one example. Do these kinds of people tend not to be involved in this country's agencies?

*Beichman:* I think the example we raised earlier holds here. I think Dan Goldin has single-handedly made a difference for technology infusion into NASA. I think he has certainly turned an agency that did not do much real high-technology, despite the hype, into one which is actually starting to do some, such that other government agencies are coming to NASA and saying, for example, "Gee, those lightweight telescopes are interesting." So I think individual people can make a difference even in a place as large as the US.

*Peterson:* And they can be part of the formal structure.

*(inaudible comments from audience)*

*Beichman:* Goldin's an anomaly.

*Allen:* Can we quote you on that? (laughter)

*Beichman:* Anomalies have great positive attributes.. (chuckles) 5 sigma, 10 sigma people, you know, are rare by definition.

*Peterson:* It's the old aphorism, "May you live in interesting times," right? And only for a short time.

*Peterson:* Bob Stachnik had to catch a plane, so I'm standing in for him. What has been said so far leads logically to one of the next questions on the list and that is: Given the current experience with international projects, what are the prospects for more? What is in the wings? Are there obvious collaborations for which the time is right?

*Shao:* I'll just repeat what Chas said, the weather is very bad right now. For NASA, large international collaborations are going to be very hard to get started in the near-term. On the other hand, things like TPF are very long-range programs and, 5 years hence, the weather will change.

*Beichman:* Regarding near-term things we're working on, like NGST, there are signed letters of agreement for collaborations between NASA and ESA. The same is true for a number of planetary missions. I guess I guess you have to put your faith in 'climate' and wait out the daily fluctuations of the 'weather'. Certainly historically the prospects for collaboration with Europe and Japan have been very, very strong. Russia has its own set of problems. The International Space Station may have diminished within NASA some of the feeling for having international collaborations on key NASA goals. Collaborations with China are

a separate story. But in places where collaborations have worked in the past, they are likely to work in the future.

*Kulkarni:* At a slightly lower level, I think these international collaborations actually have problems. I mean, their biggest virtue is what Ron Allen, or someone else here, said, that they have momentum, tremendous momentum, therefore it's hard to derail them. But it's that same momentum which can make them less than optimal, and in some cases they can have a questionable end-product. Since we're supposed to be totally frank, let's look at the MMA. There is a reasonably good design here which was done in a fairly open manner. The size of antenna one selects, or what configuration one wants: these are technical issues, but as soon as we got into negotiations with the Europeans, or the Japanese, then a kind of politics entered where management can, for example, make the antenna bigger or smaller. Hitachi can do this or that. Quickly, the issue changes and has less to do with optimizing the project and more to do with optimizing the momentum. The same thing is true of other projects like NASA's collaboration with the Japanese in X-ray astronomy. I'm not sure that the instruments put forward are necessarily the very best. So there's a difference in that there's a great deal of transference in this country, actually, which is missing in other countries to start with. The transference in this country ensures that people here may be more competitive but in the end, you actually get a better product. And marrying these high transference systems to somewhat less transference systems, I think, has its disadvantages. And particularly in areas like data rights, I think the US is actually a decade ahead of the rest of the world in our archival analysis policy. And, for example, you can see these sorts of problems with the Italian BeppoSax program and so on. It's a mixed blessing to do these international things. You really should do it only when you are totally desperate. (laughter)

*Peterson:* I suspect that view may be shared by the other side (more laughter), but let me ask. Antoine, from Europe, how are these things viewed?

*Labeyrie:* Well, yes, I agree with Shri about this, but it's true that big projects like the VLT, for example, suffered from this kind of momentum politics and I'm not sure the optimum design resulted, especially for interferometry. But there are some very positive aspects to international collaboration. The difficult thing is to make it equally rewarding for all parties, especially in the case of failures, such as an instrument failure. Insurance systems need to be devised. For commercial satellites, if the experiment fails, the rocket explodes, funding is given to the labs so they can do other things, instead. These systems need to be improved.

*Peterson:* You're saying that Europe, for example, would devise policies to share hardship on failures with the United States?

*Labeyrie:* Yes.

*Peterson:* But for serious failures, aren't there procedures in place?

*Beichman:* You know, Cluster is being rebuilt, WIRE is not, so there's a range of answers depending on, I guess, the importance of the project to the agency or agencies involved.

*Peterson:* NASA would be more the relevant agency in this case just because they are more mission-oriented.

*Beichman:* Well, yes, as opposed to NSF.

*Labeyrie:* An example I am familiar with is the HST and the FOC camera which I proposed to search for extra-solar planets in 1976, I think it went to NASA, and NASA was not very interested in these things at the time. So Europe had to build the FOC, and unfortunately the spherical aberration problem killed the coronagraph because the COSTAR corrector changed the pupil size so that it no longer fit the mask. That was a big failure for planet finding. It's likely that FOC could have found planets, but Europe was very much frustrated by its experience. There was no compensation of any kind and Europe hesitates to fund a similar coronagraph proposal for the NGST. That's just one example.

*Peterson:* Okay let's shift gears a little bit and talk about... who owes what to whom in these things. That is, if you want a big project, what does a big project most need from the community and what does a big project most owe to the community? Start with needs from the community. Chas, what do you see something like TPF needing from the people, beyond your immediate colleagues in JPL, who support it?

*Beichman:* I think the main thing that, say, a project like TPF needs is confirmation that it's a worthy scientific goal. One can talk about backroom politics and smoke-filled rooms but in the end what I see at NASA Headquarters or NSF, is people who are, in fact, enormously dedicated to getting the best science they can with the taxpayers money. All sorts of other issues come into play, but the key requirement is good science. Ultimately, the community is the guardian of that science. That's an important aspect of any big project. If it's actually going to get to first-light, or launched, or whatever, it needs a community that strongly reasserts that the project is doing something worthwhile for the astronomical community. Periodically they need to say that to more or less influential people. Even having Headquarters people told, "Yeah, this is a good project; it's good science" is essential.

*Peterson:* How about going outside of the agency?

*Beichman:* Those people who are eloquent advocates for a particular scientific project often do speak to Congress or the National Science Board, if the project needs a New Start. Those have to be people who are not typically deriving their paycheck from the project. That's a very important aspect of starting programs that the community wants moved from viewgraphs into space or onto mountain tops.

*Peterson:* And the other way around, let me ask you to speak for the community. What do you think they deserve from Chas?

*Van Citters:* Well, I guess I'd address it in terms of project, rather than agency, responsibilities. It's somewhat circular and ties back into this absolutely essential notion of constant advocacy mentioned by Chas. The project, in turn, owes its community constant, and broad, *involvement*. This is something the radio astronomers are extremely good at. NRAO partners well with university groups. It isn't just that a group of 15 people somewhere run off and spend \$200 million on their own dream. Projects also owe the community a well-understood, and prioritized, scientific capability so that hardware can be most effectively used.

*Peterson:* Are there special issues or concerns in this public policy arena?

*Labeyrie:* Searching habitable planets raises ethical questions similar to those currently debated by biologists. Searching habitable planets raises ethical questions similar to those currently debated by biologists. Observing them, with a good level of resolution, may raise possible risks, although it is hard to foresee what kind of risks. Spain's discovery of the New World caused it to destroy its own forests, which created a desert in parts of Spain. The public should be properly informed of the foreseeable benefits and risks. We must trust that it will react as positively as did our prehistoric ancestors who decided to explore the islands they could see from their coastal habitat. They probably feared the dangers implied, but still made their journeys of discovery.

What form should a debate take? should it be a formal world-wide opinion poll? should the space agencies do it? or international scientific unions such as the IAU? these are just questions, and I hope that you will have answers .

*Beichman:* I think people have worried about this in the sense that there's a lot more listening done with SETI telescopes than actual broadcasting. People actually did debate whether it made sense to make our presence deliberately known to the Universe as opposed to just listening in. I think the dangers are probably a few orders of magnitude less than in the case of transmitting our presence to whatever hungry civilizations may exist out there (laughter).

*Labeyrie:* Yes, but is it clear we should avoid public debate?

*Beichman:* NASA will face this sort of issue on a bigger scale and on a more immediate timeframe, not in worrying about things from outside the Solar System, but when we start bringing samples back from Mars. In fact there's a new appointment at NASA Headquarters: a Planetary Protection Civil Servant. This is a full-time position and there's one individual who's going to be charged with protecting life on this planet as we try to figure out how do you bring something back from Mars. I think where there are dangers of order than  $\epsilon > 10^{-10}$ , NASA is, in fact, concerned.

*Peterson:* Well, but public perception doesn't necessarily track what real dangers exist either.

*Beichman:* At least for Mars sample return, this is a recognized problem. I predict the Mars sample return people will spend far more than one Full Time Equivalent year, per year, trying to reduce risk. This is reminiscent of launch of the nuclear power source for Cassini, for which safety-related overhead was an enormous cost. And, in fact, nuclear power source fears probably rule out a large number of deep space experiments. The fact is that one cannot use a nuclear-powered spacecraft. One of the reasons we took TPF from being a 5 AU mission to being a 1 AU mission was lack of power because nuclear power sources are basically non-starters.

Yes, I think these questions do get public attention and get a reasonable amount of scrutiny when someone points out that there is a  $10^{-9}$  probability of a problem times  $10^9$  impacted. Maybe that tends to get you one person willing to worry about it.

*Johnston:* There is a lesson to be drawn here from SETI. At one time, SETI was considered a reasonable thing to do but then went through a rough spot and became unfashionable for both the Congress and the public, even though the general topic of extraterrestrial civilization was the subject of movies. We have

to be careful here that our program of looking for extra-solar planets doesn't, 10 years from now, get the connotation that SETI had. Recall that government-funding was suspended and the principals had to go out and get private funds. I think it's very, very important that this concept be sold properly and put before public and Congress in the proper way.

*Peterson:* Another question for the panel. This one is close to home. What are the mechanisms by which an individual scientist can 'get in on' a big project? Words to the wise?

*Shao:* I guess I can start. SIM is quasi-big. Actually, I guess it's a big project nowadays. As a result, SIM has issued a series of NRAs (NASA Research Announcements). There was one that was released last year and there'll be one released this year, that are mechanisms by which the project can involve the community through peer reviewed proposals for participation.

*Peterson:* There are other avenues too, though, right, Mike? For instance, there are SIM contracts for hardware...

*Shao:* That's true, although a big part of that, I mean the big contracts, of course, go to the aerospace companies. There is a big proposal effort associated with those activities. There are scientific activities, as well, that I think SIM will be contacting but those are on a smaller dollar scale.

*Kulkarni:* Yes, I approach this question in a slightly different way. That is, if I'm a young person, a post-doc, a young faculty member, and then I'm interested in an area, what is the strategy I adopt? This should be of some interest to some people in the audience. I can give you my own example.

In 1990, I decided that I wanted to switch fields and get into X-ray astronomy. I realized that it's very important to do your investment early on. So I wrote letters to the appropriate people at Headquarters saying I'd be happy to be on review panels to review X-ray satellite observing proposals. A little thing like that. There is always a desperate shortage of people willing to do this sort of work. Immediately I was on many panels. I learned a little bit about how the field works, what the instruments are, learned the language of astronomy and, more importantly, made myself known to exist in that particular field. After a couple of years on that circuit, I applied for a position on the science working group for the X-Ray Timing Explorer to see how a mission progresses, from start through launch. Then I went on to work with ASCA, the Astro-D project. I think, in fact, that strategy has been very successful. Most X-ray astronomers actually regard me as one of them now, which is, I guess, a measure of success. This progression has the effect of enlarging one's chances of being on a large mission. Right now, I am serving on the AXAF (Chandra) users group. So I think the advice to a young person is: start off small and volunteer. There's a tremendous need for you. Try to get on review panels, especially, because that's the best way to know what's going on in the field. You read proposals, you also learn how to write better proposals. When you read other proposals, you learn the language. Most importantly, this is your best way to network with the community.

*Peterson:* Is there an answer to that question for Europe, Antoine? Is it different...?

*Labeyrie:* I would say it's quite similar.

*Peterson:* Sounds like very good advice.

*Allen:* I have a question which comes back to issues we discussed when we began speaking about large projects. We focused then on large NASA missions and only touched on NSF projects. I want to get back to that because one question I would like to hear more on from the audience, or panel, is whether a case can be made for a large ground-based optical system. And if so, via what mechanism might we get started?

*Van Citters:* That's a good question!

*Peterson:* Volunteers?

*Labeyrie:* Are you talking of interferometers or large telescopes?

*Allen:* No, I'm talking about interferometers. Let's take the case of Hal McAlister. Let's ask Hal! I see him way in the back there, being very quiet. (laughter) He's been extremely successful in getting a system going at CHARA. You have all seen it. Is there a science case to be made for a CHARA-style array using 8-meter telescopes with active optics on the individual elements. Perhaps one might use very clever integrated optics for the delay lines, instead of these mechanical things that look like model trains? There's a lot that one could think of doing, but is the science case there? Do you see a way to get there from here? I'd like to hear Hal's view.

*Hal McAlister (from audience, barely audible):* Yes, I feel that there's a strong case for large ground-based interferometers. And, I think...I don't believe the scientific models for many astronomical sources have been fully tested, we have a lot to learn from the current generation of interferometers about how well they're performing. Further, the current round of interferometers serve as testbeds for the next generation. Now we should begin to answer a number of questions and this is the important question, is there a clear role for the next generation of interferometers? I believe there is. I don't think we have enough individuals in the community...

*Francois Roddier (inaudible, commenting on the role of adaptive optics)*

*Labeyrie:* Yes, years ago I proposed to ESO to expand the VLT to 27 8-meter telescopes that would be mobile along variable baselines. We continued to study such systems, but it's not clear to me whether it should be done on Earth or in space. I think the answer depends very much on the pace of progress toward space systems. If these can be built with huge mirrors, at reasonable cost within, say, 10 to 20 years, then maybe it's better to do it in space. In a sense, it's easier to operate and adjust it in space and, of course, the performance is much higher. So much of the answer depends on results from ST3. If ST3 works, then there is a tremendous push toward us doing it quickly in space, rather than on earth.

*Audience member (barely audible):* There's an intermediate case: putting things on balloons. NASA has studied this to some extent. I have been involved in these studies. You can now get some payloads to 40 kilometers for a tenth of the cost of launching satellites, and there's essentially no emission from the atmosphere shortward of the mid-infrared.  $R_0$  is many meters larger than 8 meters at that point. You still have to build the same sort of telescope we've been talking about. It has to be very light, very big, you have the same problems of optically linking elements. To the extent that space launch vehicles and operations are very expensive, it's a very interesting option. To the extent that building the

telescope and devising the technology is very expensive then you might be able to benefit from recovery and modification.

*Wes Traub (barely audible):* Yes, regarding the arguments for ground-based arrays, larger ones: I think two areas are immediate extensions of things that we are already measuring at IOTA. One is observations of dust from old stars that is being ejected back into the interstellar medium, and the other is looking at young stars for which the dust is falling in and forming planetary systems. These are both interesting areas, such that with a larger array, and some adaptive optics, in the near infrared, you can easily see that there are a lot of interesting things to be discovered there. We're very much limited now by having only 2 telescopes and not being able to study the complex structure surrounding these things. And I think that those are two areas from which we can project from results we already have in hand to establish the value of more capable arrays. The case for an array with more elements, more baselines, and somewhat larger collecting area, is immediately arguable. And, yes, you can do it from space but you can do it from the ground and you can do it now.

*Peterson:* Other comments on this?

*Johnston:* I just have one comment on what you said, Wes. The real problem that one has just now, for the case of ground-based observations, is finding the funding to create a ground-based array. The problem exists even for a nominal array, let alone a really big one. To expand your particular array, it would cost in the millions of dollars. The problem is: what agency can you go to, besides NSF, that can provide that kind of support?

*Traub: (again, barely audible)* Well, I don't know where the money comes from, but you asked what the scientific argument was. I think there are some good ones.

*Johnston:* Right, but I think, that's the major problem that I see with advanced ground-based astronomy. It's funding.

*Kulkarni:* Well, my view is that the interferometry community has been overselling its case in much the same way as the speckle community did 10 or 15 years ago. Speckle was going to do everything. Then, later, advanced optics were going to solve all our problems. For the last 10 years we've been holding championships for hyping more than we can actually deliver. I really think it's premature to make a case for a large optical array. The reason is, as was mentioned after Wayne Van Citters' talk, that we are not connecting to good questions. Most of the interferometry meetings are a bit on the boring side. We measured a star diameter. Oh, so what? The big focus in all these talks is mechanical. It's the mechanics of measurement. But what does it mean? Why should I get a star diameter to better than a certain precision? Why should I be interested in little knots of stuff flowing in or out? And what basic issues did the work address? I think we in the community really should focus on interesting questions. In fact, much of the focus is on the mechanics. Until we fix that problem, I don't think money will flow. The situation is not that there is no money, in fact, I think the case isn't there right now

*Peterson:* Or hasn't been made.

*Kulkarni:* Or hasn't been made, and articulated properly. Most of the people who are doing this are practitioners of the mechanics and it's important to

get the non-practitioners, the thinkers, into this game and make a joint case. Frankly none of the interferometry meetings are very exciting. They may be exciting for people interested in clever beam-combiners, that sort of high-tech stuff, but not on the science end.

*Larry Mertz (from audience, barely audible):* About 45 years ago, I independently asked both Walter Baade and Martin Schwartzchild whether it would be worth resurrecting the Mt. Wilson 50-ft beam and making photo-electric fringe detections. The answer I got from both of them was a resounding 'no'.

*Beichman:* You know, those Caltech astronomers are very conservative. (laughs) Right, Shri? (laughter)

*Kulkarni:* Yeah, well, we used to be... (laughter)

*Johnston:* Shri is right on a major point about VLBI. In the early days, superluminal motion pushed the whole enterprise. You would go to meetings and they sounded interesting but it was always the same old two blobs coming out of something or other. You are right, Shri, the real problem is that we're just studying simple stars here with our optical interferometry. We have to make it more interesting. It's difficult to get beyond where we are right now. It's really been difficult getting beyond stellar diameters, though we're almost beyond that now. I think that when we do get beyond stellar diameters, and we really talk more about the physics of stars, the meetings are going to be a lot better. The arrays that we have right now are, however, having difficulty getting beyond that simple first stage. If there were funding, I think you would see a lot more progress. I think things will get better over the next 5 years.

*Kulkarni:* Well, Deane, I'm doing something practical. I've really gotten interested in this issue. Over the coming year there's no question that I will go and round up the money to get a few bright theorists and manage to convince one, I won't tell you who right now, and argue, "Look, this cosmic background radiation stuff's okay, but it's passe. Get into stars!" And I think we should have some of these guys visit Caltech 2 months at a time and educate me on stars.

*Johnston:* That's good.

*Peterson:* The audience... any questions you'd like to have addressed in the last 5 minutes?

*Audience member (barely audible):* I think the scientific case can be made, though maybe it has not yet been made. But I think there is another angle on all of this that we're missing. There is an interesting list of technical hurdles along the road to achieving our astronomical objectives. Every one of them seems likely to be extremely important to corporate America. That is, interferometry, more than any other project I know of, contains very useful technology. NSF and NASA really need to understand that there will be tremendous spin-offs from being able to locate where an object is relative to something else to within half an atom. And, of course, there will be the direct products, like great telescopes, great microscopes, clever, innovative optical metrology systems. That's certainly another angle to push!

*Van Citters:* I certainly agree with that. That's an angle we've used quite often in defending the NSF budget. You have to be extremely careful about not overselling this point. For instance, to get the instrumentation budget brought

up from \$6 million a year to \$9 - 10 million a year, you come back to making the intrinsic scientific case for the science that you're trying to do, rather than relying on the possibility that it might be a cure for cancer. I'm not making fun of that notion, but that same sort of technology-based argument can be invoked by a lot of other fields, and probably more successfully. When you come up against an argument within our own directorate for instance, you're up against the Materials Research Division. They do a lot better than we in producing transferable technology, even though in our hearts we certainly believe there will be lots of spin-off from our work.

*Beichman:* One thing we have to reflect on, is that it's not even clear in which direction the vector points. Are we enabling industry to make better disk drives or is industry enabling us, as a spin-off from them, to build better interferometers? I'm not sure in which direction... (audience comment; inaudible) Yes. It's a very big arrow. I think we draw in technology much more than we emit it! Maybe in one or two very key areas we are net producers but we tend to be net-users rather than net-producers. If you come up with something really specific that astronomy has done, that's great. We should figure out how to present it and get as much credit as we can. But you have to be pretty specific and convincing when you go to Congress and say, "We astronomers invented this." Or "You need what we're peddling". If you look at the matter with a very strict regard for causal relationship, I think you'll find that number of things you can really point to is pretty small. Even expanded by one, though, that would be great. We can all benefit from being able to highlight additional specific technology transfers. But our audience is pretty smart. You run the risk of their saying "It is not true. This came out of some other lab" (laughter) or "IBM has been investing in that for 25 years and you guys are the beneficiaries rather than the creators." You really have to draw your argument very tightly and cogently to make it work.

*Shao:* I think that that is very true in astronomy in general. Many of our detectors came from the Defense Department and a lot of astronomy flowed from that theater. Adaptive optics pretty much also came from there as well. On the other hand, NASA is putting a huge amount of money into interferometry. I think it's actually enough to actually make a dent in this technological area, so I agree that in the interferometry area, there may be an argument to be made. NGST is going to be a similar case. I think too that there are other agencies now partnering with NASA to build very lightweight large telescopes. So I think that in that area it's a two-way street. It's not nearly as one-way as it used to be.

*Peterson:* Other comments? Then let's thank our panel members... (applause)

Well, that ends our scientific session. I want to thank everybody for keeping us on schedule. I think we saw a lot of talks that 'pushed the fringe without going beyond the fringe'. I want to compliment you on the quality of the talks and of the posters. See you next time around. Thank you for coming.

*Audience member:* ...and a round of applause for the Local Organizing Committee and the Scientific Organizing Committee. (more applause)

*Allen:* "May the phase be with you!"